

A Personal View of Applied Biological Research¹

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My first thought of a topic for this Presidential Address had ample historical precedent. Several Presidents have chosen, as did O.C. McBride in 1934, to describe the then current state of research in tephritid fruit flies. However, I finally decided on a less traditional subject. My perspective of the entomological world differs, I suspect, from that of the majority of you and I will use this difference to explore some of the general issues that underlie applied research.

My training before arriving at the Department of Entomology of the University of Hawaii had been at institutions where the academic disciplines were centered around what is often called basic, sometimes pure, sciences. From these early experiences I developed a view that science exists in two forms; basic, whose goal is to understand; and applied, whose goal is to exploit. Applied sciences like medicine and engineering are obviously important, but it seemed to me that in the long run basic research was more important to human progress. In this simplistic view applied science could be characterized as narrowly focused and specific in intent, often limited to a single species of interest. Its value to science as a whole would be limited since the research, even if successful, was a dead end with little application beyond the immediate goal.

On the other hand the intent of basic science is broad; the goal is to discover principles that are applicable to many species. If a specific problem is addressed at all, the solution was still less important than the developing of general principles.

The first serious challenge to my naive view of science came when a shortage of jobs in the field I had chosen forced me to work on several applied problems in mosquito biology. It soon became clear that research on specific, single species, problems with immediate practical goals did not foreclose the possibility of basic contributions. For example, C.A. Istock was able to make significant contributions to the theory of the evolution of complex life cycles (animals with different adult and immature ecological niches) from work with *Wyeomyia* mosquitoes. This gradually developing insight into the real nature of applied research was crystalized by a letter several years ago from a scientist with whom I had once worked — a world famous researcher in an area of biology renowned for its abstract mathematical methods. To my astonishment he expressed envy of the opportunity that I had to take the concepts and methods from many areas of basic science and integrate them into a project that could tackle a real world problem on fruit flies. This was doubly surprising since I had long admired him for his creativity within the world of pure science. That letter represented a turning point for me; it became certain that the ability to do effective and innovative work at an interface between applied and basic science is not a vulgar skill. In fact, there is an additional ironic twist to this comparison of basic and applied science. The basic scientist is often frustrated in not being able to directly utilize his or her skills on practical problems. I think that there is evidence for this view from within the current biotechnology revolution. The large numbers of molecular biologists who have eagerly become involved in enterprises to apply the tools of genetic engineering and monoclonal antibodies to practical prob-

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lems are only partially lured by the prospects of financial gain. I think that these new enterprises give some basic scientists the chance to work on practical problems with immediate, direct application to the world's many problems.

This train of thought leads me to a series of questions which speak to our basic ordering of priorities, as individuals, and in our various roles in society. These are fundamental questions of who one is, how we view the world, our philosophy of life and work.

The purpose of science

In general the views of each individual scientist and non-scientist as to the purpose of science can be placed somewhere on a continuum. At one end is the view that the sole purpose of science is service to mankind in a practical sense — increasing the happy and productive lifespans of the greatest numbers of persons. The opposite extreme is that scientific thinking involves the highest exercise of human faculties and while benefits, direct or indirect, are important, they are not the *raison d'être*.

The nature of science

This question is related to the first, but focuses instead on the constituency, if any, to which the scientist answers. In agricultural areas it is often implied that the needs and priorities of farmers are paramount. However, some persons feel that an area of study is inevitably corrupted by goal orientation. The most extreme formulation of this view was once expressed to me by a very distinguished colleague at this University. He claimed that the only types of extramural research grants that should be accepted are those that allow the investigator to turn in any direction that seems appropriate at the time without regard to a product, goal, or application.

My view, like that of the majority of working scientists falls somewhere between these extremes and can be summed up in three thoughts.

1. There is a constituency for scientific research, but the group is usually broader than at first glance. In particular the constituency to whom we should owe allegiance is often broader and somewhat different from the funding or directing group.
2. A primary role of science is service, but this may involve long-term, broad benefits which may not be immediately clear to the constituency.
3. Science should be abstract and dispassionate even when the goal is a problem of emotional impact — starvation, disease. This should be a deliberate strategy chosen to do the job right; so as to disassociate the work from the goal.

This brings us to two critical questions; who decides what research is done and funded?; who profits from the economic benefits of the research?

Who Decides

This decision has obviously become one for all the political forces within society to make. No longer does the public eagerly accept the word of the expert speaking within their area of expertise. The recent flurry over an eradication program for fruit flies in Hawaii is an excellent example. The plan was blocked by a public perception that on balance it did more harm than good. This happened despite expert testimony from the agricultural research establishment as to the best course of action.

Money for pure or basic research from, e.g., NSF or NIH, is almost always finally justified by the practical benefits that may eventually develop. There would be little money for billion dollar atom smashers or space programs, for example, if it were definitively stated that no practical (or military) benefits would ever come from this research.

Who profits

Sometimes the scientists themselves — consultant work for private gain has always existed, usually on a small scale. However, there is currently a powerful, and probably irreversible trend, coming somewhat late to Hawaii, for the potential financial rewards from research to be shared amongst the scientists and the universities. As this trend develops we will be forced to directly face the important issue of the place for secret and proprietary research at the University.

I will now discuss these issues in a specific context taken from the area I know best — fruit fly research. The central question I will pose is — is fruit fly research important and in what sense? The importance of an area of research can be evaluated from several aspects.

1. Economic impact. In Hawaii, there are other problems which in terms of dollar value may appear to warrant more of the limited agricultural research budget e.g., thrips.

2. Political influence of constituency. There is no need to expand on this point since you are all aware of this factor and realize that budgets are not necessarily distributed in proportion to the crop value.

3. Value of knowledge gained to other research, programs. Some types of research obviously have much greater applicability to other problems.

4. The importance of a research area can also be judged on the basis of its benefits to the scientists themselves. The "hotness" of the work can increase job opportunities both as academics and as consultants to private companies. This can translate directly into higher salaries and indirectly into higher probabilities of successful grant applications.

Let me conclude this discussion with some "how to" rules, based on my experience, for a strategy of effective "innovative" applied research.

RULE 1 — Use many approaches.

Much of innovative applied research involves the application of pioneer technologies to new species. The scientist is betting on long shots, most of which will fail. Therefore, one should always have several "irons in the fire" to increase the overall probability of at least one success.

RULE 2 — Remember the "tooth decay paradox".

This is meant to describe a phenomenon whereby the success of the research decreases the demand for the worker's expertise. This is more a psychological than a scientific problem. At first sight it may appear frivolous, but it involves our basic conceptions of our work as scientists to ask whether a particular area of research has lasting importance. We all need some confirmation of the lasting value of our work which extends beyond the immediate goals. If the medfly should be eradicated, workers in this species do not want to feel that their research has become irrelevant. As a personal strategy we should always have some component of basic research to our work that will survive the immediate problem.

RULE 3 — Play to your strengths.

This rule is straightforward enough — a clear example is the fruit flies in Hawaii. We have 4 species established in a small area, a natural laboratory to study and test strategies for control.

RULE 4 — Be careful playing to your strengths.

This rule may be even more important than the preceeding one. There are 4 species of fruit flies here which cannot be studied anyplace else in the USA. This is an effective point to make in one's strategy for grants, but does not in and of itself

lead to effective research. Our strongest assets can also create problems. An object lesson of which I am aware can be drawn from the case of a department at a major American university which is located near a unique and fascinating ecosystem. Over the years the faculty built up great depth and expertise in studying this ecosystem. It turned out that this emphasis on their strengths had essentially stifled other areas and the department had need of a major rebuilding program. Our advantages here in Hawaii must always be kept in balance — a good case for say, an arctic biologist is easy to make here.

RULE 5 — Remember all you have learned from your work and from that of others.

This is to emphasize the need for the broadest based education as a student so that one can recognize and understand the advances that will be needed in one's own work.

RULE 6 — Forget-all, deliberately, if necessary.

This rule was especially important to me when I first came here to work with fruit flies after several years study of mosquitoes. I wasted time trying to make the genetics and ecology of fruit flies fit that of *Aedes* mosquitoes. It is only when the painful realization came that some of my experience was not directly transferable that I began to make progress with my work here. This Rule 6 is especially important since our natural reluctance to start over is very strong. I know of at least one case where problems arose because a whole system of analysis was transferred from one group of species to another and the obvious differences glossed over.

In summary, effective and innovative applied research requires skills as valuable and as rare as those of any area of human endeavor and will require more and more cultivation in the future.